

ORNITHOLOGICAL JOURNALS RECEIVED.

- The Auk, Vol. XXXI, April and July, 1914.
 Bird-Lore, Vol. XVI, Nos. 2, 3, 4. 1914.
 Bluebird, Vol. VI, Nos. 10 and 11. 1914.
 The Condor, Vol. XVI, Nos. 2, 3, 4. 1914.
 The Oregon Sportsman, Vol. II, Nos. 4, 5, 6, 7. 1914.
 The Oriole, Vol. II, Nos. 1, 2. 1914.
 The Taxidermist, Vol. 2, No. U. 1914.
 Our Feathered Friends, Vol. I, No. 4. 1914.

CORRESPONDENCE

A REJOINDER.

BY T. C. STEPHENS.

There appears in a recent number of the Auk* a criticism by W. L. M. of some work which has appeared from time to time by students of the Macbride (Iowa) Lakeside Laboratory. Inasmuch as the present writer is largely responsible for this work, and inasmuch as similar work may appear in the future, it becomes a duty to ascertain to what extent the criticisms are grounded.

The criticism is directed wholly at such parts of the work as relate to the food of nestling birds, a field which seems to be guarded zealously by the critic as the peculiar domain of the Biological Survey.

Let us examine specifically some of the objections raised. He charges the workers with "over-enthusiasm" (a statement rather too vague to detain us), and goes on to say that it is a grievous fault "to publish identifications that could not possibly have been made under the circumstances."

Truly, this is a bold and sweeping accusation. Upon what does our rash reviewer base his confidence? W. L. M. further says, "Now the positive identification of a mosquito, and the distinguishing of the house and stable flies, two obscurely marked species of the same family, require far closer and more definite observation than could possibly be made on specimens in process of being fed to nestling birds."

This criticism is directed at Gabrielson's work on the catbird (Wils. Bull., XXV, Dec., 1913, pp. 179-180), where, in Table III, 99 "Flies" were recorded as being fed to the young over a period of ten days; and in which the text says "The flies were mostly fish flies, though house and stable flies were also noted."

* The Auk, XXXI, July, 1914, pp. 420-421. "W. L. M." presumably stands for W. L. McAtee, of the Biological Survey, but inasmuch as his name does not appear on the editorial staff, and not having been introduced by the Editor of The Auk, the writer regrets to be compelled to refer, in the present note, simply to the initials as signed.

Now the nest of the catbirds was in a bush located on a steep hillside. The blind being higher, enabled the observer to see over and around the bushes. Flies swarmed about on the foliage of these bushes, and the observer in the blind could see the catbird capture them and feed them to the young birds in the nest. A number of these flies were caught and submitted to an entomologist from Ames College, who was teaching that subject at the laboratory, and who named the flies as above. The paper did not state that every fly fed to the young birds was recognized as to kind. In the tables II and III they are simply listed as "Flies." The enumeration in the text may have been based upon specific data, or it may have been a general estimate based upon memory, and still be an accurate statement.

The original statement is perfectly safe, and scientifically accurate, notwithstanding the obstinate misinterpretation by the critic.

Now, in regard to the mosquitoes, which are also denied by W. L. M. in the statement above quoted. The one mosquito recorded in the catbird paper (page 179) was observed under the following circumstances, as communicated to me by the author of that paper: "The old bird was on the nest, and I was in the blind. As it was only 8:00 a. m., a few mosquitoes were still about. One in the blind buzzed around my face, and I struck at it with my hand. It flew out of the peep-hole, and as I idly followed its flight it lighted on a leaf within six or eight inches of the nest. The old bird immediately snapped it up and fed it to one of the nestlings."

With reference to the mosquito records in the Yellow Warbler study (Wils. Bull., XXV, June, 1913, p. 55), I can only call the reader's attention to the fact that as the observer sat in the blind, the nest was almost as close to his eyes as is a newspaper while being read—not over two feet away. The bill of a Yellow Warbler is only 3.5 mm. wide at its base, while the terminal third of it is not over a single millimeter in width. Thus even the body of a mosquito could scarcely be entirely concealed in the bill of such a bird. I am well aware that it is almost a waste of time to be discussing the question whether a mosquito was actually seen or not; but I would simply remark that when the possibility of it is so evident, it would seem that the critic is rather forcing an issue. When we admit the possibility of seeing one mosquito, the repetition of it, even to sixty-five times, should give us no greater concern.

Our captious reviewer displays a lack of knowledge of this kind of field work, and its methods, when he questions an observer's ability to count 5, 6, or 7 Mayflies in the beak of a Brown Thrasher at the nest. May I be permitted to call attention to a few elementary facts?

The date on which these seemingly large numbers of Mayflies were recorded was June 28, at which time the Mayfly swarms were at their maximum. In the evening dense clouds of them filled the air, and during the day the grass was full of them. Frequently the old birds fed in the

grass in close proximity to the nest, where they were under observation. In a few instances the Mayflies were counted as they were being gathered. On this date 244, counted, Mayflies were fed to the young.

Usually the old bird pauses an instant at the nest before feeding, during which time there is an excellent opportunity for counting. In fact, Mr. Gabrielson tells me that this summer, while watching a Rose-breasted Grosbeak's nest, he was able, by making a slight noise, to hold the male on the edge of the nest for three minutes, by the watch, while trying to determine the contents of its beak. I wish I might assure the critic that it is not surprising for the bird to have so many Mayflies in its beak; neither is it, under the circumstances, particularly difficult to count that number of them.

The ants may be discussed in a similar way. I think no more than three ants were recorded at any single visit. In all of these records, it is understood, I had supposed as a matter of course, that the number recorded were seen and counted; but it was not claimed, nor was it intended to convey the impression, that no more were in the bird's bill. For instance, if the old bird visits the nest with a beak full of ants, and the observer could distinguish the bodies of three individuals, it would be ridiculous to assume that no more than three were in the bird's mouth. This is so elementary!

It will be found that in Gabrielson's report on the Brown Thrasher study, in Table I, the plus sign was frequently used to indicate that a certain number of individual insects were recognized out of a larger number. In this report (Wils. Bull., XXIV, June, 1912, p. 84) there will be found the following statement: "It will be noticed in the tabulated data that the number of insects was not always determined exactly, but was entered in this manner, '6+ Mayflies,' etc. In all such cases the minimum number was used in computing the tables. As all of the persons who assisted were cautioned especially to note the number of insects exactly, it is safe to assume that if there be any error in the data it is in having recorded too few insects, rather than too many."

The reviewer then believes he has given sufficient illustrations of the inaccuracy of the work to demolish it completely, and proceeds with this *ex cathedra* pronouncement: "It should be recognized that reporting on the food of nestling birds on the basis of field observation is work for accomplished entomologists, not for amateur ornithologists," with emphasis, perhaps, on the "amateur." Of course, no one will dispute this statement, although the work is more likely to be done by an ornithologist who knows some entomology, than by an "accomplished entomologist." The only fault with such a remark is the animus revealed by it, which does not beget confidence or friendliness. The reviewer is expected to give more conclusive proof of inaccuracy before indulging in such caustic comment.

The very excellent pioneer report on the nest study of the Chipping

Sparrow by Dr. C. M. Weed* is cited as a model, because of the indefiniteness of identification of the food fed to the young sparrows. This nest of the chippy was "near" a window, from which it was watched; but nothing further was stated to enable one to know whether the distance was two feet or ten feet, or whether it was watched through an open or closed window. It is only fair to the authors of this paper to quote from page 109 as follows: "The precise determination of the most of the food brought was, of course, impossible, the observations having been undertaken especially to learn the regularity of the feeding habits of the adult birds." Since this study was not undertaken for the purpose of determining the nature of the food, it hardly seems proper, in fairness to the author, to set it up as an example of this line of work.

The reviewer's proposal to tie bags over the anal orifices of nestling birds for the purpose of collecting the excreta will be highly amusing to anyone who has noticed young birds in the nest. However, any suggestion coming from so well qualified a critic deserves attention, and the writer will endeavor to try out this new method at some future time.

As another suggestion that the authors of the several papers reviewed may have been deceived in their observations the reviewer has said, "A great many birds feed by regurgitation and the food is at no time visible." We take it that the reviewer here has in mind passerine birds, since no other order was involved in the discussion.

In our studies on the passerine birds we have succeeded in following the feeding of at least one out of a brood, from the moment it left the egg until it left the nest, in the cases of the yellow warbler, the catbird, and the meadowlark (report on the last having not yet been published); and in each of these instances there has been no feeding by regurgitation. This is known simply from the fact that the food has been visible in the bird's bill. It is quite possible, of course, that regurgitation may be found to be practiced by certain passerine birds, such as the flycatchers and the grosbeaks, and it is just such questions which can be settled by field observation. (I am not now considering the carrying of berries in the throat of a waxwing as coming under the definition of regurgitation.)

The reviewer's confession of limited experience in field work of this kind is sufficient reason in itself to make him more cautious of such vigorous, though quibbling, criticism.

It would seem that he is very skeptical of the value of field observations on the food of nestling birds in any case. It is to be assumed that he relies wholly upon the examination of stomach contents. But there are limitations to that method also. The examination of a stomach will give, at best, the story of only three or four hours of the bird's life. Even with the food mass in a watch glass, some of the material must be macerated beyond recognition. What is unrecognizable cannot

* Weed, Clarence M. An Observation on the Feeding Habits of the Chipping Sparrow. N. H. Agric. Exp. Sta. Bull. 55, 1898, pp. 101-110.

be taken into account, except as "unknown" or as "miscellaneous." If the tables or diagrams do not show this must we not conclude that the writer has discarded the unidentified material? Wilcox,* who examined over 200 stomachs of the robin in one year, says: "The determination of insect remains in the stomachs of birds is a very difficult and perplexing task, and one which is not all pleasant, since nearly all the material is in the very worst condition imaginable, and mutilated and partly digested fragments of several species of insects being mixed up in utter confusion. The elytra, mouth parts and tarsi are, of course, usually left to tell their tales, as are also the harder parts of all other insects, snails, myriapods and the seeds of the various fruits; but the soft bodied larvae and earthworms are too often macerated almost beyond recognition." (p. 118.)

Too often the adherent of stomach examination publishes only his percentage results, without the detailed data upon which his percentages are based, which are necessary in a strictly scientific piece of work.

Most ornithologists will concede that field observations on the food of birds possess certain advantages; those who have had much practice in this method will understand that it yields results with far greater accuracy than its critics are ready to admit.

No field worker, I presume, would claim that field observations alone would give us a full knowledge of the economic status of a species. It will be claimed, however, that such observations contribute to such knowledge very largely, if not with parity, in comparison with other methods. Furthermore, this method is not destructive of life, which would become a fact of importance in the study of any rare species. It is not particularly reassuring to read the boast of having killed so many thousands of nestling birds in order to determine what their food had been for the last two or three hours. The writer recognizes that under certain circumstances it may be justifiable, but nevertheless, in the judgment of many this criticism will apply to the stomach method.

It would seem, when a careful review is made, that the hypercritical apostle of stomach examination ought to be more cautious whither he slings. To paraphrase the reviewer's closing remark, what is needed above all on the part of iconoclastic reviewers is more certainty and less quibbling, and more hard work in the field and laboratory that there may be developed an appreciation of the difficulties to be encountered in productive effort.

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* Wilcox, E. V. Bull. 43, Ohio Agric. Exp. Sta., 1892, pp. 115-131.